

S T A V I E R

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Dec. 24, 1949

Dear Josh -

Your letter arrived just as I was about to come down here for Christmas, and since it couldn't very well be answered out of hand I brought it along for further consideration.

In the first place, let me reassure you about Ellis Englestad's personality. He's a very nice kid, with a naivete that Mike and I find rather charming. We also live in crowded conditions, and Ellis has never proved difficult to get along with.

My reservations in suggesting that you offer him space stem rather from my uncertainty about his scientific abilities. He's a hard worker, conscientious, ~~but~~ enthusiastic, with a respectable though not brilliant academic record, but I think he lacks that Luria once called "the constructive twist". About this, however, I may well be mistaken; it's quite possible that the fault was mine, and that I never provided the stimulus necessary to develop him. In order to explain this, I shall have to give you a brief synopsis of the development of his problem. When I came to Berkeley, he was vaguely interested in bacterial genetics, and so I suggested that he play a bit with the itaconate mutant of *P. fluorescens*. I'd been thinking about Lwoff's work on the acetoinic mutant in *Moraxella*, and the apparent occurrence of this mutation in the absence of growth, so it seemed a good start to study the relationships between growth + mutation with the itaconate mutant. The outcome of this work you've seen. At this point, Ellis became interested in the possible induction of mutations with formaldehyde, a problem which rather bored me and which I consequently pushed-aside. He tried the effect of it on the itaconate mutant, and found an astounding increase in the mutation rate, and at once took off after this like a hound after a hare. He hoped to be able, in view of the known effects of formaldehyde on proteins, to provide the beginnings of an explanation of its mutagenic action in chemical terms. I was still notably unenthusiastic, and told him that I

considered it a wildly over-ambitious project, but he went ahead anyway and spent almost a year on it without getting anywhere. During this time I simply let him go without attempting to influence his work in any way. Some months ago he gave it up, much chastened by the experience, and has now returned to the problem of the mechanism of itaconate + mesaconate utilization by itaconate + mesaconate mutants respectively.

Looking back over the business, I feel that I mismanaged the direction of his work rather badly. What he really needed, in psychological terms, was a powerful father-figure who would tell him what to do; one of those people who has daily conferences with his students at which the progress of the work is followed & guided step by step. This is a role that I'm temperamentally incapable of playing. I'm willing to offer hints of possible experiments, which a student can accept or reject as he pleases, and to criticize a hypothesis once formulated, but not to direct the whole affair. My attitude has been that anyone else should be able to see a problem as well as I can. I now realize that Ellis expected and needed more than this non-directive approach. Consequently I feel that someone else might be able to bring him out where I failed to do so. If you're willing to try, I would be very pleased, although I don't guarantee the results. If he came to your lab, at worst he'd provide a useful pair of hands, and as I said in the beginning he's a nice enough kid.

I'm sorry not to be more dogmatic in my advice, but this the matter is.

Best wishes to you both

Roger